**Climate
of the Past
Discussions**

Interactive comment on "The influence of the circulation on surface temperature and precipitation patterns over Europe" *by* P. D. Jones and D. H. Lister

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The authors thank both reviewers for their thoughtful and constructive reviews. We have taken all the comments on board. Figure 6 was reproduced incorrectly in the version reviewed. Instead of 12 panels, only one panel was shown. Both reviewers realised this, but also realised what should have been there.

Review by Jucundus Jacobeit

This paper is an important contribution to the issue of circulation-climate relationships, based on a recent daily mean SLP classification and daily European station time series for the 1911-2000 period. It indicates that warming within some of the circulation types

is an important factor in context of the general long-term warming across Europe. The focus of this paper is on the entire continent of Europe, therefore it is necessary for defining dominant wet, dry, warm and cold circulation types to use a rather low threshold for the number of stations with significant anomalies in precipitation or temperature (at least half of the stations). In a more regional context stronger thresholds or criteria could be used, but I think looking at the whole European area is also a feasible and intriguing approach. I would suggest only some slight modifications to this interesting paper listed below as two major and some minor comments.

No comment needed

Major comments:

Those readers who are not familiar with the results of Philipp et al. (2007) will have some problems to understand large parts of Fig. 6 results (p. 544), since only 3 circulation types (CT) are reproduced in Figs. 3-5, for the other ones mentioned on p. 544 there is no idea of particular pressure distribution patterns. In contrast to another reviewer who suggested to reproduce further CTs (which can be seen in Philipp et al. 2007), I would suggest to describe shortly those CTs which are mentioned as dominant wet, dry, warm and cold patterns. This would help to understand these characteristics from a dynamical point of view.

According to the abstract the authors consider "whether the long-term warming across Europe is associated with more favourable weather types or related to warming within some of the weather types". The latter aspect is well investigated in the paper, however, the former aspect is not represented correspondingly (apart from some citations of Philipp et al. (2007) on p. 545). I would suggest to refer to Fig. 1 from which some informations can be drawn concerning frequency changes of dominant warm and cold CTs.

The omission of the complete Figure 6 possibly led to the first few sentences. Following the other reviewer, it seems simpler to put all 34 examples (from which Figs 3-5 were

drawn) as supplementary information. So we have chosen this option. Readers will be able to see the pressure maps for each type and their responses in surface temperature and precipitation data. The Supplementary Information has been put on a special page on the CRU web site. The complete Figure 6 illustrates which CTs are dominantly warm or cold, wet or dry. When combined with Figure 1, frequency changes in the types that dominate in one aspect can be seen.

Minor comments:

p. 536, l. 20/21: additionally to Yarnal (1993) the authors could mention a recent review paper by Huth et al. (2008):

Huth R., C. Beck, A. Philipp, M. Demuzere, Z. Ustrnul, M. Cahynová, J. Kyselý, and O.-E. Tveito (2008): Classifications of atmospheric circulation patterns: recent advances and applications. Annals of the New York Academy of Sciences 1146: 105-152.

Reference has been added

p. 537, l. 11: concerning the issue of frequency-related and within-type-related changes, the authors explicitly cite two papers (Osborn and Jones 2000; van Olden-burgh and van Ulden 2003) – another one dealing with this issue (though with monthly resolution) is mentioned among the references (Beck et al. 2007) but not considered in the text.

The paper has been referred to at this point.

p. 537, l. 27: when discussing the influence of large-scale circulation types on regional climate, a station-based index of the NAO is not the best example, large-scale pressure anomaly patterns would be better.

This is a debatable point. It is only possible to make this point with station-based indices.

p. 538, l. 1/2: an example that more local pressure gradients increase the explained

variance of regional temperature variability has been given by Jacobeit et al. (Climatic Change 48, 2001, p. 233) comparing correlation coefficients of central European tem-peratures with an index of the NAO and with a central European zonal index (the latter giving higher values than the former).

Reference added together with some text.

p. 539, l. 16: the criterion for determining the number of types in Philipp et al. (2007) is not just simply based on explained variance, but on several conditions for a dominant loading (see p. 4075).

Text clarified – as it is a detail, we have simplified and referred specifically to Phillipp *et al.* (2007).

p. 539, l. 20: the "basic type" in this clustering context could be addressed more clearly as the centroid pattern.

Text added

p. 539, l. 24: what do you mean with "continuous nature of typing" for simplified techniques?

A sentence explaining this has been added

p. 540, l. 25: it would be useful to have exact numbers of series for the different 30-year periods.

This has been added.

p. 545, l. 22-24: for regression models there is also a significant (95% level) in-crease during spring (see Tab. 5 in Philipp et al. 2007). The slightly negative trend in circulation-related temperature in spring is only true for composite models, but not for regression models (same Table).

Text modified

Figs. 3-5:

- Why do you select examples for summer, spring and autumn, but not for winter?

- Why do you select just these examples and not other ones?

We thought that there was only room for three. We have included all in the supplementary material.

- Which level of significance is used in all these figures?

95% - this has been added

- Line 3 of the figure caption: the solid lines are negative (not positive) anomalies, and correspondingly the dotted lines are positive (not negative) anomalies.

Well spotted, this has been changed

- *The contour lines in the precipitation, temperature and DTR panels depict only little additional information.*

Agreed – but we have left them in, as they had been included.

- *In the caption of Fig. 3 the sub-period (1911-1940) has to be mentioned.*

Done

Fig. 6:

- Obviously this is only one out of 12 panels (4 seasons, 3 variables) mentioned in the text, 11 panels are missing.

See beginning statement

- Which level of significance is used in all these panels?

Again 95% - added to the text

- *The reversed red/blue for precipitation should also be mentioned in the legends for precipitation (not only at the end of the figure caption).*

This will be clear once the whole figure is there.

Anonymous Reviewer (2)

This reviewer summarised the paper and pointed out that only 3 of the 34 possible maps were there. They also pointed out that Figure 6 had only one panel and 12 were expected.

We will put all 34 panels in as supplementary material

Figure 6 will be complete and we will add some text.

Minor Points

Page 537, line 1 - done

Page 542, line 6 – done

Page 544 line 27 – done

Figure 3-5 – with these being as supplementary material users will be able to enlarge them to get more detail.

Figure 6 – see earlier discussion.

Anonymous Reviewer (Third one)

I have no fundamental objection to this manuscript. The paper is rather descriptive, and presents an excerpt of an obviously thorough analysis. My remarks follow: The goal of the paper is "to assess whether the long-term change in temperature over Europe [: : :] is a result of a change in the mix of CTs or is a result of within-type

changes in the CTs" (p. 538, l. 14). Note that those mechanisms are not necessarily the only ones that can cause long-term change in temperature. Moreover, I do not think that the authors address this question in their manuscript. Instead, they assess the response (of temperature), assuming that the driver is the atmospheric circulation (note that temperature variations also affect surface pressure, which is not discussed here). Although I believe that it is important and interesting, it is different from the original goal of the manuscript.

The reviewer seems to have misunderstood what our aims in the paper were, and consequently doesn't think that we have addressed them. We have modified the text to try and make what we are trying to do clearer. Throughout this review there is clearly a difference in terminology. The temperature affect on surface pressure is considerably smaller than the circulation effect on temperature. Surface pressure data are adjusted for this temperature effect anyway.

The authors should cite work by other researchers who have tackled this kind of problem [e.g., Corti et al., 1999; Michelangeli et al., 1995; Palmer, 1999; Robertson and Ghil, 1999; Yiou et al., 2007].

Pascal Yiou was involved in the EMULATE project. We had several discussions in the project on the differences between 'Weather Regimes' and 'Circulation Patterns'. The two are not the same, but they are related. It is possible that they just relate to different terminology. The main difference with these 'Weather Regimes' papers is that they don't consider patterns of temperature or precipitation change specific to individual CPs (or WRs). Some of the papers referred to also only consider the WRs for surface temperature or precipitation extremes, so don't consider the whole distribution of days. Finally, all of the papers either use Reanalysis data or MSLP/700hPa heights, so only consider periods from the late 1940s onwards. So, in our view, none are relevant to the work we are doing. We referred to those that we consider are relevant – and have added those suggested by Jucundus Jacobeit, as they were relevant.

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The description of the Monte Carlo method (p. 540, l. 26) is rather vague (I gather that what the authors did is close to a bootstrap method). How (and why) does this procedure allow for an assessment of significance?

The description of how we determine significance was clear for the other reviewers! It may be related to bootstrap approaches. We though the method we have used was so straightforward, it didn't need more detail.

The authors have a better command of English than me, but they should avoid colloquial phrasing (haven't, doesn't, can't: : :) in the manuscript. What do the authors mean by "weekly" (p. 545, l. 22)? That sentence is rather strange and might need to be simplified to non native English speaking readers. Figure 6 (p. 555) seems incomplete. Its legend mentions at least two panels. I see only one, entitled 'summer_prec'.

As native English speakers, we think we are best to determine what is good and what is poor English. The word 'weekly' was misspelt. The text has been changed there anyway, so the word is no longer used. It should have been 'weakly'. Figure 6 was incomplete – see earlier responses.

I believe that he deserved the H. Oeschger medal, but a bibliographic search cannot do harm.

The paper isn't related to the Hans Oeschger medal, which was awarded in 2001. I've been asked regularly by Denis-Didier Rousseau to contribute a paper, and this is the first time that I've had something that wasn't already earmarked for another journal. I don't know what a bibliographic search will achieve.

Interactive comment on Clim. Past Discuss., 5, 535, 2009.

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